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gene G should be stably related to its new position in chromosome C' it must be held incorporated by force  $F'_g$  and not by force  $F_g$  as is the case. Added on to the original problem of the interchange of the genes is now the second and equally imposing problem of the interchange of the forces subsequent to the interchange of the genes. An actual bodily interchange of the forces seems impossible in view of the assumptions we have had to make as to their nature and action. The transformations would then have to be accomplished by some transmutation *in situ*. It is evident that no internal autonomous change short of a complete and absolute mutation of force  $F'_g$  in chromosome C into  $F_g$  and simultaneously of  $F_g$  in C' into  $F'_g$  would suffice. But we have no precedents for assuming such reciprocal mutations, and if we had, we could have sidetracked this whole machinery by applying this reciprocal transmutation idea to the genes and thereby solved the first problem in such a way that the second could not arise. Instead of localizing the cause of the reciprocal transformations of the forces in the forces themselves, one might transfer it to the genes; *i. e.*, one might endow the genes with the power of causing reciprocal transformations of the forces rather than empower the forces to transmutate of their own accord. While this form of the transmutation idea carries something of an air of plausibility, it can not be taken as more than an attempt at formal escape from the difficulty—a lifting of one's self by one's boot straps that makes more demand on credulity than, for example, one would in assuming crossing over offhand as a specific property of genes which needs, as support, only such formal explanation.

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## ON THE PROBABLE ERROR OF MENDELIAN CLASS FREQUENCIES

AN old friend of geneticists who dislike excessive calculation has recently been attacked by Pearl,<sup>1</sup> viz., the familiar formula,  $\sigma = \sqrt{npq}$  for the standard deviation of a Mendelian class frequency. He proposes to substitute a more refined but much more complicated method, originated by Pearson. In a Mendelian illustration he obtains a result which differs by over 40 per cent.

<sup>1</sup> Pearl, R., "The Probable Error of a Mendelian Class Frequency," AMERICAN NATURALIST, Vol. LI, pp. 144-156, 1917.

from the usual. This seems to indicate that the old method is wholly inadequate, but further examination shows that the difference is not due so much to method as to the fact that Pearl has calculated something with a different significance from the usual probable error. A cross of Mendelian heterozygotes (Blue Andalusian fowls) gave three classes of young in the numbers 14:33:11. Expectation is 14.5:29:14.5. Pearl assumes that a first sample of 58 has given exactly expectation and then calculates the quartile deviations for each class in a second sample of 58. The results are given as 3.13 for the heterozygous classes, 3.55 for the homozygotes which indicate an excellent fit of observation to expectation. By the usual method, if a first sample of 58 had given exactly 14.5 black chicks and nothing were known of any theoretical expectation, the probable error in a second sample of 58 is measured by the probable error of differences. The probable error of either sample as given by the formula  $.6745\sqrt{npq}$  is 2.22. The probable error of differences by the usual formula  $.6745\sqrt{\sigma_1^2 + \sigma_2^2}$  is 3.15. This does not differ appreciably from Pearl's quartile of 3.13. Neither of these methods, however, gives what we really wish to know, the closeness of fit to Mendelian expectation. We have a theoretical expectation which is not based merely on a particular sample of 58, but which should hold with increasing accuracy the larger the first sample taken. With an infinite first sample, the formula given by Pearl reduces to the usual one,  $.6745\sqrt{npq}$  giving a quartile of 2.22. This is less lenient to the discrepancy between expectation and observation than the first result, but the fit is still not bad. In a second illustration which is given, we do have two samples and no theoretical expectation suggested. The usual method of comparing samples of different sizes would be to find the standard deviation of differences on a percentage basis. The percentage standard deviation for a sample of  $n$  individuals is  $\sqrt{pq/n}$ , for a sample of  $m$  individuals is  $\sqrt{p'q'/m}$  and for differences is  $\sqrt{(pq/n) + (p'q'/m)}$ . The expected standard deviation of a sample of  $m$  individuals is, however,  $m\sqrt{(pq/n) + (pq/m)}$  if  $p$  and  $q$  are based merely on the first sample as in Pearl's illustration. The formula given by Pearl for the standard deviation rapidly approaches this form for large values of  $m$  and  $n$ . Following are the results given by the long method, by an approximation given by Pearl and by the usual one just cited.

	Long Method	Approximate Method	Usual Method
Median .....	83.53	83.95	83.71
Lower quartile.....	75.61	75.84	75.64
Upper quartile.....	91.82	92.06	91.79

The usual method gives substantially the same result as the long one and a better result than the approximate method. From the nature of experimental work, great refinement in statistical treatment is often a waste of effort, and without questioning the value of Dr. Pearl's suggestion in cases in which the greatest accuracy is warranted it appears that the simple formula is still adequate for most practical purposes.

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### CHARACTERS INDICATIVE OF THE NUMBER OF SOMATIC CHROMOSOMES PRESENT IN ÆNOTHERA MUTANTS AND HYBRIDS

THE pollen grains of 28-chromosome *Ænothra Lamarckiana gigas* de Vries were long ago shown (Lutz, '09)<sup>1</sup> to be characteristically 3+ -lobed (chiefly 4-), instead of 3-lobed, as in *O. Lamarckiana* and other diploid forms. Gates has since contributed much to our knowledge of this subject. Recently Bartlett ('15)<sup>2</sup>, in discussing the 3+ -lobed condition of the pollen of 28-chromosome *O. stenomeris* mut. *gigas*, stated that these 3+ -lobed grains "are larger than the triangular grains of the type" (*O. stenomeris*). It may be added that the largest, best-appearing 3- of tetraploid forms in general, is larger than the typical, best-appearing 3- of diploid, and the largest, best-appearing 3+ - of the former, larger than the typical, occasional 3+ - of the latter. Smaller 3- and 3+ - grains are found in the pollen of both, but they are rarely perfect-appearing, and it is doubtful whether slightly imperfect-appearing grains are capable of functioning. A careful examination of the adult characters of a form, together

<sup>1</sup> Notes on the first generation hybrids of *Ænothra lata* × *O. gigas*, *Science*, N. S., 29: 263-267. Gates (Pollen development in hybrids of *Ænothra lata* × *O. Lamarckiana*, *Bot. Gaz.*, XLIII, 81-115, Feb., 1907) had earlier observed 3+ -lobed grains in the pollen of a triploid form.

<sup>2</sup> "The Mutations of *Ænothra stenomeris*," *Amer. Jour. Bot.*, 2: 100-109.